Does social capital mitigate agency problems? Evidence from Chief Executive Officer (CEO) compensation

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Does social capital mitigate agency problems?
Evidence from Chief Executive Officer (CEO) compensation

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Abstract

We find that social capital, as captured by secular norms and social networks surrounding corporate headquarters, is negatively associated with levels of CEO compensation. This relation holds in a range of robustness tests including those that address omitted variable bias and reverse causality. Additionally, social capital reduces the likelihood that firms make opportunistic option grant awards that unduly favor CEOs, including lucky awards, backdated awards, and unscheduled awards. Social capital also lessens the accretive effect of CEO power on CEO compensation. These findings indicate that social capital mitigates agency problems by restraining managerial rent extraction in CEO compensation.

JEL classification: D23, J33, J44, M12, Z13
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1. Introduction

Individuals, and even organizations, are susceptible to social influences in the geographical areas in which they reside.[1] In particular, research across various disciplines in social science, including Coleman (1988), Putnam (1993), Fukuyama (1995), and Buonanno, Montolio, and Vanin (2009), has provided consistent evidence that social capital, as captured by strength of secular norms and density of social networks in geographical areas, discourages opportunistic behaviors, encourages cooperation, facilitates economic transactions, and produces positive economic outcomes. Guiso, Sapienza, and Zingales (2011) provide a review of this literature and conclude that economic research in social capital would benefit by emphasizing norms that prescribe "the set of values and beliefs that help cooperation" (i.e., cooperative norms). Following this practice, recent research finds that social capital matters in the corporate setting (e.g., Jha and Chen, 2015). More specifically, managers of corporations with headquarters located in areas with higher levels of social capital tend to constrain self-serving corporate practices that could benefit shareholders at the expense of other stakeholders (e.g., Hasan, Hoi, Wu, and Zhang, 2017a). Building on these studies, we explore whether social capital in local geographical areas surrounding corporate headquarters mitigates agency problems in resident corporations.

Agency problems can result in managerial rent extraction in CEO compensation, leading to opportunistic pay practices that unduly favor the CEO and higher pay levels (e.g., Yermack, 1997; Bertrand and Mullainathan, 2001; Bebchuk, Fried, and Walker, 2002; Bebchuk and Fried,

[1] For instance, Hong, Kubik, and Stein (2004, 2005) find that social interactions in local geographical areas promote stock market participation and affect trades of money managers residing in the areas. Pirinsky and Wang (2006) find that social interactions in local geographical areas surrounding corporate headquarters contribute to comovement in the stock returns among resident corporations. Hilary and Hui (2009) and McGuire, Omer, and Sharp (2012) find that religious adherence surrounding corporate headquarters promotes conservative corporate investment and reduces the incidences of financial reporting irregularities.

2003). Rent extraction behaviors are contradictory to the prescribed values of cooperative norms. Moreover, dense social networks help to communicate and enforce the attendant code of conduct associated with cooperative norms (e.g., Coleman, 1988; Fukuyama, 1995; Uzzi, 1996; Woolcock, 1998). Therefore, managers of firms with headquarters located in areas with higher levels of social capital should anticipate greater marginal costs for exploiting rent extraction opportunities (e.g., Elster, 1989; Posner, 2000) when compared to their counterparts with headquarters located in areas with lower levels of social capital. Accordingly, we conjecture that social capital surrounding corporate headquarters mitigates managerial rent extraction in CEO compensation, leading to negative associations between social capital and opportunistic pay practices that unduly favor CEOs and resulting in lower levels of CEO pay.

We focus the main analysis on CEO total compensation and equity-based pay (i.e., stock and option awards) because these measures reflect broad consequences of managerial rent extraction in CEO compensation (e.g., Bertrand and Mullainathan, 2001; Morse, Nanda, and Seru, 2011). Additionally, following Narayanan and Seyhun (2008), Lie (2005), and Bebchuk, Grinstein, and Peyer (2010), we use incidences of unscheduled awards, backdated awards, and lucky awards to capture specific consequences of opportunistic timing in CEO option grant awards that unduly favor CEOs.

Our empirical analyses explore the effects of social capital in US counties on both CEO pay levels and opportunistic timing in CEO option awards of local corporations headquartered in the county. The social capital measure is based on data that reflect county-level secular influences arising from social networks and cooperative norms (e.g., Rupasingha, Goetz, and Freshwater, 2006), where cooperative norms are non-religious social norms that emphasize cooperative behaviors (e.g., Knack, 1992). The social capital construct could reflect influences arising from county-level factors such as economic development, infrastructure quality, labor market quality, diversity, political preferences, and religious adherence (e.g., Putnam, 1995; Alesina and La Ferrara, 2000, 2002; Rupasingha, Goetz, and Freshwater, 2006). Accordingly, we include a range of county-level measures to isolate the effects of income level, population growth, population density, latitude, longitude, distance to river, age, educational level, nonwhite population percentage, relative strength of Democratic/Republican party as captured by electoral outcomes, and religious adherence as captured by the fraction of a county's population that claims affiliation with an organized religion. We also include a range of firm-level variables to control for differences in firm attributes, CEO attributes, and corporate governance that can potentially affect CEO compensation; and, we include year and industry fixed effects in the empirical models.

Using a comprehensive sample of annual compensation data drawn from the Compustat ExecuComp database in the two decades of 1993–2014, we find negative and statistically significant relations between social capital and the levels of CEO total compensation and CEO equity-based pay, holding all the aforementioned factors constant. When the level of the county's social capital increases from the 25th percentile to the 75th percentile in our data, our coefficient estimates translate into a reduction of 17.31% (0.1731=$0.452 million÷$2.612 million) in CEO equity-based compensation and a reduction of 7.97% (0.0797=$0.367 million÷$4.606 million) in CEO total compensation, on average, respectively.

We perform a range of tests to allay concerns of omitted variable bias. First, we identify preferences for redistribution, income inequality, and metropolitan setting as omitted variables that could cause a negative association between social capital and CEO pay and create spurious associations in our regression models (Alesina and Angeletos, 2005; Yamamura, 2012; Alesina and La Ferrara, 2002; Glaeser, 2010; Rupasingha, Goetz, and Freshwater, 2006). Our results are robust and the estimated coefficients on social capital do not attenuate significantly when we control for differences in these factors in the regressions. Additionally, our results are robust in a range of sensitivity analyses that isolate the effects of other unknown (and therefore omitted) factors affecting CEO pay. In particular, our results are robust when we use region fixed-effect regressions and state fixed-effect regressions, indicating that unknown time-invariant region-level or state-level factors do not explain our findings. Our results also hold when we use the average of CEO pay in other firms headquartered in the same county to isolate the influences of unknown county-level factors affecting local pay practices. Lastly, our results hold when we include additional controls to isolate the influences of anti-takeover protection and general managerial skills.

Even so, reverse causality could make it challenging to infer a causal relation between CEO pay and social capital. To mitigate this concern, we use an epidemiological approach (e.g., Guiso, Sapienza, and Zingales, 2006; Algan and Cahuc, 2010) to construct instruments for social capital based on inherited cultural preferences, namely, cultural preferences that people in a given US county inherited from their ancestral origins. These instruments are based on Hofstede's scores of national culture, namely, masculinity-femininity and power distance. Our results are unchanged in instrumental-variable two-stage regressions where we use these inherited cultural preferences as instruments for social capital.

Further, our results are also unchanged when we use an alternate social capital measure and alternate samples including using a propensity score matched sample to estimate the model.

We also find similar results using an alternate empirical design that isolates the effects of over-time changes in social capital on over-time changes in CEO compensation. In this analysis, we use a difference-in-differences method to compare changes in CEO compensation surrounding corporate headquarter relocation events that change the level of social capital that firms face.

Consistent with Adams, Almeida, and Ferreira (2005), we find that, on average, powerful CEOs—those CEOs who simultaneously hold the titles of chairperson of the board of directors and president of the company—extract higher compensation in both total pay and equity-based pay holding other factors constant. Given our conjecture, one would expect that social capital moderates the positive power-pay relation. Indeed, we find that social capital significantly reduces the positive power-pay relation, suggesting that social capital moderates the accretive effect of CEO power on CEO compensation.

The aforementioned results provide tentative evidence that social capital restrains managerial rent extraction in CEO compensation setting. To pin down this inference, we follow Lie (2005), Narayanan and Seyhun (2008), and Bebchuk, Grinstein, and Peyer (2010) to examine the effects of social capital on incidences of CEO option grant awards that likely embody consequences of managerial rent extraction, including unscheduled awards, lucky awards, and backdated awards. An unscheduled award is an award that follows no particular timing pattern. A lucky award is an award with a grant date that has the lowest price in the grant month. A backdated award is an award that is associated with abnormally higher stock price returns surrounding the award date. (We provide detailed definitions of each of these awards in Section 6.)

We employ two alternate samples for these additional analyses. The first sample is drawn from the Thomson Financial Insider Trading database and covers the time period of 1993–2006 in the main analysis. It contains 12,812 firm-years with at least one unscheduled option grant award to the CEO in the year. We use this sample to explore the effects of social capital on incidences of unscheduled awards and backdated awards, respectively. The second sample contains 9,872 firm-years between 1996 and 2005 for which around 14% of the observations include at least one lucky award in the year. This sample is derived from the sample analyzed by Bebchuk, Grinstein, and Peyer (2010). We find that social capital reduces the likelihood that a firm makes at least one lucky award to its CEO in a given year; social capital also reduces the likelihood that a firm makes at least one backdated award (or at least one unscheduled award) to its CEO in a given year. These additional findings provide direct evidence that social capital limits opportunistic timing in CEO option grant awards.

Taken together, our results indicate that social capital surrounding corporate headquarters limits opportunistic timing in CEO option grant awards that unduly favor CEOs, reduces the influences of CEO power in setting CEO pay, and reduces the levels of equity-based pay and overall compensation for CEOs. These findings provide novel evidence that social capital mitigates agency problems in corporations by limiting the consequences of managerial rent extraction in CEO compensation setting.

To date, researchers have treated CEO compensation and social capital as disparate constructs; and, there is little effort to explore the systematic linkage between them. Building on the insight that cooperative norms and social networks in geographical areas affect individual behaviors and corporate decisions (refer to Footnote 1 and Section 2 for more in-depth discussion), we provide evidence that such secular, social influences also mitigate agency problems in CEO compensation setting. This particular insight bridges the research in social capital and CEO compensation. Specifically, it shows an unexplored economic benefit of social capital for shareholders of publicly listed corporations through the mitigation of agency conflicts in CEO compensation, extending the results of Knack and Keefer (1997) and Guiso, Sapienza, and Zingales (2004, 2008) on social capital and the results of Yermack (1997), Bebchuk, Fried, and Walker (2002), Bebchuk and Fried (2003), Lie (2005), and Bebchuk, Grinstein, and Peyer (2010) on managerial rent extraction in CEO pay.

More broadly, our findings add to a better understanding of how secular norms and networks surrounding corporate headquarters affect publicly listed corporations. Hasan, Hoi, Wu, and Zhang (2017a, b) find that such secular influences constrain corporate tax avoidance and facilitate debt contracting. Jha and Chen (2015) find that such secular influences engender trust between the firm and external auditors, resulting in lower audit fees. We find that, holding other factors constant, such secular influences mitigate agency problems in CEO compensation. We view all of these findings as complementary: they indicate that secular norms and networks in geographical areas could constrain corporate practices that are incongruent with the prescribed values and standards associated with the prevailing, attendant norms in the areas.

Prior research on executive compensation, including Hartzell and Starks (2003), Morse, Nanda, and Seru (2011), Custodio, Ferreira, and Matos (2013), has focused on the influences of firm-level and executive-level factors. Despite voluminous research, we still know little about whether and how influences arising from social institutions, particularly social influences arising from institutions in local geographical areas, affect executive compensation. As such, our findings are informative in that they provide fresh evidence to fill this particular gap in the executive compensation literature.

2. Prior literature and hypothesis development

Agency problems can result in significant managerial rent extraction in CEO compensation, leading to opportunistic pay practices that unduly favor the CEO and higher pay levels (Yermack, 1997; Bebchuk, Fried, and Walker, 2002; Bebchuk and Fried, 2003; Lie, 2005; Bebchuk, Grinstein, and Peyer, 2010). This section provides a review of the social capital literature and develops the hypothesis that expounds the deterrent effect of social capital on opportunistic behaviors, including managerial rent extraction in CEO compensation.

2.1. Social capital and opportunistic behaviors

Prior studies have deployed various operating definitions of social capital (Rupasingha, Goetz, and Freshwater, 2006). Despite that, a common approach advocated by Coleman (1988), Putnam (1993), Knack and Keefer (1997), Woolcock (1998), and Guiso, Sapienza, and Zingales (2004) is to define social capital as an environmental factor that captures the confluence of effects arising from the strength of social norms and the density of associational networks in a geographical community. Taking this logic one step further, Guiso, Sapienza, and Zingales (2011) argue that economic research on social capital should focus on norms that prescribe "the set of values and beliefs that help cooperation" (i.e., cooperative norms). This is reasonable since cooperative norms tend to constrain narrow self-interest (Knack and Keefer, 1997), limit opportunistic behaviors in transactions (Coleman, 1988), and help to overcome the free rider problem by increasing trust (Guiso, Sapienza, and Zingales, 2008, 2011). Heeding this advice, we adopt the approach that identifies cooperative norms and social networks as key constituents of social capital (e.g., Knack and Keefer, 1997; Woolcock, 1998).

There is evidence that individuals residing in communities with higher levels of social capital—namely, people residing in communities with strong cooperative norms and dense social networks—are less likely to engage in opportunistic, self-serving behaviors. For instance, Lederman, Loayza, and Menendez (2002) and Buonanno, Montolio, and Vanin (2009) find that social capital deters individuals from engaging in criminal behaviors. Bjornskov (2003) finds that individuals in higher-social-capital countries are less likely to accept bribes or bribe others. Posner (1980) finds that dense social networks in African villages reduce the opportunistic behaviors of villagers. Across these studies, the intuition is that individuals perceive opportunistic behaviors as contradictory to the prescribed values associated with cooperative norms while dense social networks help to communicate and enforce the attendant code of conduct through more frequent, repeated social interactions. Uzzi (1996), Fukuyama (1995), and Fischer and Pollock (2004) argue that frequent, repeated social interactions amount to repeated games over time that cultivate a code of conduct that deters opportunistic behaviors. Coleman (1988, p. S100) argues that "social capital exists in the relation among persons" because social networks provide efficient information sharing and better communication and enforcement of the prescribed norms. Accordingly, one would expect that dense social networks intensify the costs that individuals anticipate for perpetrating opportunistic behaviors. On one hand, these costs include external social sanctions (Coleman, 1988) such as social ostracism (Uhlaner, 1989) and stigmatization (Posner, 2000). On the other hand, since individuals have a great need to maintain a moral self-concept (Mazar, Amir, and Ariely, 2008), these costs also include psychic costs produced by heightened negative moral sentiments such as guilt and shame, which could arise even if the actual behaviors are unobserved (Higgins, 1987; Elster, 1989). Consequently, individuals in communities with higher levels of social capital, as captured by strong cooperative norms and dense social networks, should anticipate higher marginal cost for perpetrating opportunistic, self-serving behaviors. Therefore, one would expect that social capital deters individuals from engaging in opportunistic behaviors.

2.2. The effect of social capital on managerial rent extraction in CEO compensation

If social capital deters individual opportunistic behaviors, social capital should also limit opportunistic corporate practices because corporate decisions are made by managers (e.g., Bertrand and Schoar, 2003) and corporate managers are susceptible to social influences surrounding corporate headquarters (e.g., Hilary and Hui, 2009). Evidence is consistent with this conjecture. For instance, Hoi, Wu, and Zhang (2018) and Hasan, Hoi, Wu, and Zhang (2017b) find that firms headquartered in US counties with higher levels of social capital—hereafter, high-social-capital firms—undertake fewer corporate activities that could benefit shareholders at the expense of other stakeholders. Hasan, Hoi, Wu, and Zhang (2017a) find that high-social-capital firms pay more corporate taxes, indicating that social capital in US counties helps to cultivate a local environment that deters corporate tax avoidance practices, which are widely perceived by people in the society outside of the corporate sector as contradictory to the prescribed values and standards of cooperative norms. Collectively, these findings imply that managers anticipate higher costs for undertaking opportunistic corporate dealings when their firms are headquartered in areas with higher levels of social capital. However, we still know little about whether social capital mitigates agency conflicts between shareholders and managers; and, more specifically, we still know little about whether social capital constrains managerial rent extraction in CEO compensation.

CEOs have significant influences in setting their own pay and they tend to use their influences to exploit more opportunistic pay practices that unduly favor themselves and extract higher levels of compensation.[2] Accordingly, Bebchuk, Fried, and Walker (2002) argue that many equity-based compensation practices can be construed as outcomes of managerial rent extraction. Bebchuk and Fried (2003) advocate the argument that views executive compensation as an agency problem.

Nevertheless, based on the insights from the aforementioned social capital literature, one would expect that managers anticipate higher marginal costs for perpetrating opportunistic behaviors, including those that pertain to their own compensation, particularly when their firms are headquartered in high-social-capital areas. Consequently, just as social capital deters opportunistic individual behaviors and corporate practices, social capital should also deter managerial rent extraction in CEO compensation. Moreover, since outcomes of CEO compensation practices are observable and, in fact, they are under heavy public scrutiny and regulatory oversight, it is likely that the purported disciplinary effects of social capital could be significant. We formulate the prediction from this perspective as a testable hypothesis as follows:

> *Hypothesis 1*. Managerial rent extraction in CEO compensation is negatively associated with social capital surrounding corporate headquarters.

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[2] Bertrand and Mullainathan (2001) find that CEO total compensation responds to random firm performance shocks beyond the CEO's control, implying that CEOs are rewarded for luck. Garvey and Milbourn (2006) find that CEO pay is more sensitive to good luck (i.e., more sensitive to industry or market benchmarks when such benchmarks are up) than to bad luck. Morse, Nanda, and Seru (2011) find that CEOs use their influence to rig compensation contracts by shifting weights toward performance measures that are doing better. Grinstein and Hribar (2004) find that managerial influences drive the cash bonus payments that CEOs receive for completing mergers and acquisitions. On the other hand, Yermack (1997) provides evidence that CEOs use their influence over timing of stock option awards to capitalize on impending improvements in corporate performance. Lie (2005) and Heron and Lie (2009) find that stock returns are generally negative before option grants but they are generally positive after option grants, suggesting that option awards could be backdated. Bebchuk, Grinstein, and Peyer (2010) find that CEOs receiving option grants at the lowest price of the grant month also have higher total pay, even after controlling for other factors that explain CEO compensation.

3. Research design and summary statistics

This section introduces the empirical measures that we use to capture broad consequences of managerial rent extraction in CEO compensation and social capital, describes the baseline regression model, explains the sampling procedure, and presents the summary statistics.

3.1. Measures of CEO pay

CEO total compensation is the sum of salary, bonus, equity-based compensation, and various other forms of compensation including deferred compensation, contribution to retirement plan, change-in-control payments, perquisites, and other personal benefits. Equity-based compensation, which comprises a large portion of a CEO's total pay, includes values of option grant awards and values of restricted stock awards. Prior research suggests that CEOs wield significant influences in setting their own pay and these influences could exemplify in more opportunistic pay practices that unduly favor the CEOs and higher levels of CEO compensation (Yermack, 1997; Bertrand and Mullainathan, 2001; Bebchuk, Fried, and Walker, 2002; Bebchuk and Fried, 2003; Lie, 2005; Garvey and Milbourn, 2006; Bebchuk, Grinstein, and Peyer, 2010; Morse, Nanda, and Seru, 2011). Accordingly, we focus our analysis on the levels of CEO total compensation and CEO equity-based pay because these measures reflect broad consequences of managerial rent extraction. The variable, *Total pay* (*Equity pay*), is the natural logarithm of one plus CEO total compensation (CEO equity-based compensation) as reported in the Compustat ExecuComp database for a firm in a given year. Although not our focus, we also provide evidence based on cash and other compensation for the CEO. *Bonus* (*Salary*) is the natural logarithm of one plus CEO cash bonus payment (CEO cash salary) as reported in the Compustat ExecuComp database for a firm in a given year. *Other pay* is the natural logarithm of one plus the sum of long-term performance payout in incentive plans (before 2006), deferred compensation (after 2006), contribution to retirement plan, perquisites, change-in-control payments, other personal benefits, etc. To mitigate the influence of extreme observations, we winsorize these CEO compensation variables and all other continuous variables in the study at the 0.5% and the 99.5% levels.

3.2. Social capital measure

We define social capital as joint influences arising from social networks and cooperative norms in US counties. The Northeast Regional Center for Rural Development (NRCRD) at the Pennsylvania State University provides data that capture cooperative norms and social networks in all US counties in the years of 1990, 1997, 2005, and 2009, respectively. Rupasingha, Goetz, and Freshwater (2006) describe these data in detail. The data contain information on voter turnouts in presidential elections (*Pvote),* response rates in US census surveys (*Respn*), total numbers of ten types of social organizations (*Assn*), and total numbers of nonprofit organizations (*Nccs*).

The *Nccs* measure reflects individual participation in tax-exempt nonprofit organizations with a domestic focus. The *Assn* captures individual participation in a range of social organizations including bowling centers, physical fitness facilities, public golf courses, sports clubs, civic associations, business associations, political organizations, religious organizations, and labor organizations. These measures are particularly relevant for our analysis because they reflect repeated, face-to-face social interactions and connections both within and across networks that are likely to promote cooperation and reinforce the attendant norms of the networks (Coleman, 1988; Putnam, 1995). On the other hand, the *Pvote* and the *Respn* measures are empirical proxies that reasonably capture the manifestations of cooperative norms (Alesina and La Ferrara, 2000) since there are no legal or direct material incentives to vote or to take a census survey (Knack, 1992; Guiso, Sapienza, and Zingales, 2004; Funk, 2010).

Following Rupasingha, Goetz, and Freshwater (2006) and Hasan, Hoi, Wu, and Zhang (2017a, b), we construct the test variable, *Social capital*, using the first principal component from a factor analysis based on *Pvote*, *Respn*, *Nccs*, and *Assn*. We can only directly estimate *Social capital* in 1990, 1997, 2005, and 2009. Accordingly, we follow Gompers, Ishii, and Metrick (2003) and Hilary and Hui (2009) to backfill data for the missing years using estimates of *Social capital* in the preceding year in which data are available. For example, we fill in missing data from 1998 to 2004 using *Social capital* in 1997.

3.3. Baseline regression models

We use the following empirical specification to test the implications of our hypothesis:

$$\textit{Total pay}_{t+1} \text{ or } \textit{Equity pay}_{t+1} = f\,(\textit{Social capital}_t, \textit{CEO attributes}_t, \textit{firm attributes}_t, \textit{county attributes}_t, \textit{industry dummies}, \text{ and } \textit{year dummies}). \quad (1)$$

$\textit{Total pay}_{t+1}$ and $\textit{Equity pay}_{t+1}$ are as of year $t+1$. $\textit{Social capital}_t$ and control variables are as of year t. The extant evidence indicates a range of firm-level factors that potentially affect CEO compensation (e.g., Hartzell and Starks, 2003; Custodio, Ferreira, and Matos, 2013). Accordingly, we include a range of firm attributes to control for the effects of size, risk, leverage, asset tangibility, and growth opportunities; and, we control for the effects of CEO tenure, CEO age, and institutional ownership (both level and concentration). We also include accounting-based (return on assets) and stock-based (raw stock return) firm performance measures in the empirical models to control for the influences of managerial ability and luck on CEO pay.

The social capital construct could reflect influences arising from county-level attributes such as economic development, infrastructure quality, labor market quality, diversity in race, political preferences, and religious adherence (e.g., Putnam, 1995; Alesina and La Ferrara, 2000, 2002; Rupasingha, Goetz, and Freshwater, 2006). Accordingly, we include a range of county-level measures to control for the effects of income level, population growth, population density, latitude, longitude, distance to river, age, educational level, nonwhite population percentage, relative strength of Democratic/Republican party as captured by state election outcomes (Rubin, 2008), and the fraction of a county's population that claims affiliation with an organized religion (Hilary and Hui, 2009). By doing so, one is more confident that the estimated coefficients on *Social capital*$_t$ reflect residual variation in local social environment that is captured by the social capital construct and not explained by these other factors. Finally, we include dummy variables to control for two-digit Standard Industrial Classification (SIC) industry effects and year effects in the regression models. The industry dummies are intended to isolate the effect of regulatory environment on CEO compensation (Smith and Watts, 1982). The Appendix presents detailed definitions and constructions of all these variables. Hereafter, we omit the subscript to ease the exposition and refer to the aforementioned regression models as the baseline models.

3.4. Sampling procedure and summary statistics

We estimate the baseline models using a data set constructed with information obtained from various sources. We begin with annual executive compensation data from the Compustat ExecuComp database in the two decades of 1993–2014 for which complete financial and stock price information is available in the Compustat and CRSP databases and institutional ownership data are available in the Thomson Reuters Ownership database. This limits our sample to those firms in the Standard and Poor's (SP) 1,500 universe in the period 1993–2014. We extract corporate headquarter locations using historical information in electronic 10-K filings from the Securities and Exchange Commission (SEC) Electronic Data Gathering, Analysis, and Retrieval (EDGAR) database. We use the resulting state and county names or Federal Information Processing Standards (FIPS) codes of each firm's headquarter location to match social capital data from NRCRD and other data on county-level demographic factors from the Bureau of Economic Analysis and the US Census Bureau. The final data set contains 2,396 unique firms and 22,246 firm-years in the period 1993–2014 for which all requisite data for regressions are available.

Table 1 presents sample statistics for all variables used in the baseline regressions. On average, the levels of total compensation and equity-based compensation are \$4.6 million and \$2.6 million, respectively. These sample statistics are in the range of those reported in prior studies. For example, Custodio, Ferreira, and Matos (2013) report mean values of \$4.5 million and \$2.5 million for total compensation and equity-based compensation, respectively. As expected, equity-based compensation constitutes a large proportion of the CEO's total pay, roughly 56.7% in our sample (0.567=\$2.612 million/\$4.605 million). There are significant variations in the levels of social capital in our data; the standard deviation of *Social capital* is 0.834 with a mean of -0.441 and the corresponding interquartile range is between -1.127 and 0.168. Much of the variation in *Social capital* in the data comes from the differences in social capital across counties at a point in time; although the overtime changes in *Social capital* are nontrivial in our data, they dwarf the cross-sectional variations in *Social capital*.

[Insert Table 1]

4. Social capital and the levels of CEO compensation

4.1. Baseline regression results

Table 2, Panel A, presents results of the baseline models using Ordinary Least Square (OLS) regressions with county-level clustered standard errors. The dependent variables are *Total pay* and *Equity pay*. The coefficients on *Social capital* are negative and significant at the 1% level. They are -0.064 and -0.147 in the regressions of *Total pay* and *Equity pay*, respectively. These findings are consistent with our hypothesis.

[Insert Table 2]

The relative magnitudes of the coefficients are informative; they show that social capital has a larger effect on the level of equity-based compensation. This empirical regularity is in line with our expectation as existing findings indicate that equity-based pay practices are particularly vulnerable to managerial influences (e.g., Yermack, 1997; Bebchuk, Fried, and Walker, 2002). Based on the *Social capital* estimates in the baseline regressions, an interquartile increase in *Social capital* from the 25th percentile to the 75th percentile in our data would reduce *Equity pay* by 0.190 (0.190=0.147×1.295) and it would reduce *Total pay* by 0.083 (0.083=0.064×1.295). Since the mean values of CEO equity compensation and CEO total compensation are $2.612 and $4.606 million in our sample, these results imply that an interquartile increase in *Social capital*, on average, reduces *Equity pay* by about 17.31% and it reduces *Total pay* by about 7.97%, respectively.[3] By way of comparison, Morse, Nanda, and Seru (2011) find that a one-standard-deviation increase in

[3] For *Equity pay*, an interquartile increase in *Social capital* reduces the level of CEO equity compensation to $2.160 million (where $2.160 million = exp(ln(1+2,612)-0.190)-1)×$1,000) relative to the mean level of $2.612 million, reflecting a reduction of 17.31% in CEO equity compensation. Similarly, an interquartile increase in *Social capital* reduces CEO total compensation to $4.239 million (where $4.239 million = exp(ln(1+4,606)-0.083)-1)×$1,000) relative to the mean level of $4.606 million, reflecting a 7.97% decline in CEO total compensation.

CEO power index, as captured by CEO personal influence over the board of directors, raises total CEO compensation by about 4.5% in their sample. Based on our estimates, a one-standard-deviation increase in *Social capital* reduces total CEO compensation by about 5.17%.[4]

Although not our focus, Panel B reports coefficients from baseline regressions using other compensation variables as the dependent variables, namely, *Bonus*, *Salary*, and *Other pay*. For brevity, we report the estimates on *Social capital* only. In these regressions, the coefficients on *Social capital* are negative in general, but significant at the 5% level only when *Bonus* is the dependent variable. The coefficient on *Social capital* is insignificant at the conventional level when either *Salary* or *Other pay* is the dependent variable.

4.2. Effects of preferences for redistribution, income inequality, and metropolitan setting

The association between CEO compensation and social capital could be spurious if our baseline models omit factors that correlate with both CEO compensation and social capital. To allay this concern, we identify preferences for redistribution, income inequality, and metropolitan setting as specific omitted variables that could cause a negative association between CEO pay and social capital. Our results are robust and the estimated coefficients on social capital do not attenuate significantly when we control for these factors in the regressions.

[4] These data might overstate the effect of social capital on CEO compensation because Morse, Nanda, and Seru (2011) use a within-firm setting and we do not. To allay this concern, we perform a horse-race by re-estimating the regressions after adding a dummy variable, *CEO power*, to the baseline models. *CEO power* equals one if a firm's CEO also serves as the chairperson of the board and president of the company in a given year; CEO power equals zero otherwise. Based on the estimated coefficients from this specification, a one-standard-deviation increase in *Social capital* reduces CEO total compensation by 5.22% and the corresponding change in *CEO power* increases CEO total compensation by 4.75%. The results from this regression are not tabulated.

Preferences for redistribution vary systematically across countries and across local communities within a nation (Alesina, Glaeser, and Sacerdote, 2001; Alesina and La Ferrara, 2005). Yamamura (2012) finds that people in local communities in Japan with higher social capital also exhibit stronger preferences for redistribution. Individuals tend to have strong redistribution preferences when they have low social mobility, anticipate low future income prospects, and doubt the fairness of social competition, namely, when people are dubious that it is hard work, rather than luck, birth, connections, and/or corruption, that affects income (Benabou and Ok, 2001; Alesina and Angeletos, 2005). Accordingly, preferences for redistribution could be negatively associated with levels of CEO pay.

We use survey data from American National Election Studies (ANES) to measure people's preferences for redistribution. The data are based on each respondent's answer to question VCF0809 from the ANES surveys: "Some people feel that the government in Washington should see to it that every person has a job and a good standard of living. (Suppose these people are at one end of a scale, at point 1.) Others think the government should just let each person get ahead on his/their own. (Suppose these people are at the other end, at point 7. And, of course, some other people have opinions somewhere in between, at points 2, 3, 4, 5, or 6.) Where would you place yourself on this scale, or haven't you thought much about this?" We code the data so that a higher number means a respondent is more favorable to redistribution.

In the sampling period for our analysis, ANES conducted biennial surveys in US counties from 1994 to 2012, except for the years of 2006 and 2010. However, ANES did not survey all the counties and ANES conducted only one survey in 53% (or 206 out of 390) of the counties in our sample. This makes it difficult to obtain a meaningful time-varying county-level measure for redistribution preferences. Accordingly, we use the variable, *County redistribution preferences*, to capture time-invariant county-level preferences for redistribution. *County redistribution preferences* is the mean of ANES respondent data in a given county across all years in which ANES conducted a survey in the county.

We re-estimate the regressions after adding this omitted variable to the baseline models. Table 3, Panel A, presents the results. For brevity, we report the estimates on *Social capital* and the respective omitted variable only; we continue this reporting practice for the remainder of Table 3. Across the models, the estimates on *County redistribution preferences* are negative but insignificant. In contrast, the estimates on *Social capital* remain negative and significant at the 1% level.

[Insert Table 3]

Income inequality is negatively associated with social capital (e.g., Alesina and La Ferrara, 2000; Putnam, 2001; Rupasingha, Goetz, and Freshwater, 2006). However, the pay levels of local CEOs residing in a county are likely to widen the income gap in that county, possibly resulting in a positive association between income inequality and CEO pay. Accordingly, we use Gini coefficients to capture income inequality at the county-year level. The variable, *Income inequality*, is based on annual estimates of Gini coefficients for US counties from 2006 to 2014 as reported in the American Community Survey (Variable B19083). We backfill data for the missing years using estimates of Gini coefficients in 2006.

Firms located in metropolitan areas could enjoy significant agglomeration benefits including lower transportation costs, lower communication costs, and increased efficiency (e.g., Ciccone and Hall, 1996; Glaeser, 2010). This could result in higher CEO compensation. However, Rupasingha, Goetz, and Freshwater (2006) find that metropolitan setting (i.e., major cities and their surrounding suburban localities) is associated with lower levels of social capital even after controlling for other demographic characteristics such as education, age, income, etc. Accordingly, we use the variable, *Metro*, to isolate the effects associated with metropolitan setting. *Metro* is a dummy variable that equals one if a firm's corporate headquarter is located within a 250-kilometer radius of a metropolitan statistical area with more than one million residents according to the census of 2010; it equals zero otherwise.

Table 3, Panel A, presents the results from the regressions after adding the respective omitted variable to the baseline models. Across the models, the estimates on *Income inequality* and *Metro* are insignificant, except for the estimate on *Metro* in the regression where *Total pay* is the dependent variable. Nevertheless, estimates on *Social capital* remain negative and significant at the 5% level or better across the models, suggesting that the baseline regressions are not significantly plagued by these omitted factors.[5]

4.3. Effects of other omitted variables

Still, our empirical models might omit unknown (and therefore omitted) regional or state characteristics that affect social capital and CEO pay. We use region fixed effect regressions and state fixed effect regressions to examine the influences of unknown region-level and state-level factors that are relatively stable overtime. Table 3, Panel B, presents the results. In these models, we re-estimate the regressions after adding region fixed effects and state fixed effects to the baseline models, respectively. Across the models, the estimates on *Social capital* remain negative

[5] The estimates on the omitted factors are generally insignificant. This empirical regularity might be due to the fact that we have already included a range of county-level control variables in the baseline model.

and significant at the 5% level or better, suggesting that the baseline models are not plagued by significant omitted time-invariant region-level or state-level factors.

Both social capital and levels of CEO compensation are relatively stable and sticky over time. As such, county fixed effect and firm fixed effect regressions would likely absorb most of the variations in these variables, making it extremely difficult to detect a relation between social capital and CEO pay even if one exists. Accordingly, we employ alternate approaches to establish the robustness of our findings with respect to omitted county-level and firm-level factors.

For omitted county-level factors, we use the variable, *Other total pay* (*Other equity pay*), which we calculate using the mean value of *Total pay* (*Equity pay*) for other S&P 1500 firms headquartered in the same county in a given year. The idea is to capture influences of unknown county-level factors affecting CEO pay that change over time. We add *Other total pay* and *Other equity pay* to the baseline model separately and re-estimate the regressions accordingly. Table 3, Panel C, presents the results. Across the models, the estimates on *Social capital* remain negative and significant at the 1% level when the dependent variable is *Total pay*. The estimate is significant at the 10% level when *Equity pay* is the dependent variable (p-value = 0.08). These findings suggest that the baseline models are not plagued by significant unknown county-level factors affecting CEO compensation.

For omitted firm-level factors, we use the variable, *E-Index*, to capture influences of corporate governance arising from anti-takeover provisions and charter amendments that firms adopted.[6] The idea is to capture influences of firm-level corporate governance quality on CEO pay.

[6] *E-Index* is an index that captures managerial entrenchment proposed by Bebchuk, Cohen, and Ferrell (2009). It is the total number of anti-takeover provisions a firm has in a given year, including staggered boards, limits to shareholder bylaw amendments, poison pills, golden parachutes, and supermajority requirements for mergers and

Bebchuk, Cohen, and Ferrell (2009) find that *E-Index* is monotonically associated with economically significant reductions in firm valuation and large negative abnormal returns, suggesting that anti-takeover provisions reflect poor corporate governance. For omitted executive-level factors, we use the variable, *General ability*, to capture influences of general managerial skills.[7] Custodio, Ferreira, and Matos (2013) find that *General ability* is positively and significantly related to CEO pay levels.

We add *E-Index* and *General ability* to the baseline models separately and re-estimate the regressions accordingly. The remainder of Table 3, Panel C, reports the corresponding results. Across the models, the estimates on *E-Index* and *General ability* are positive and significant at the 1% level, possibly reflecting the influences of firm-level anti-takeover protection and executive-level general managerial ability on CEO pay. Nevertheless, the estimates on *Social capital* remain negative and significant at the 1% level in most of the regressions, except in the *Equity pay* regression where *General ability* is added to the model in which the estimate on *Social capital* is negative and significant at the 5% level. These findings suggest that the baseline models are robust to influences of these firm-level and executive-level factors.[8]

4.4. Sensitivity to alternate measure and alternate sampling method

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charter amendments. We thank Professor Bebchuk for providing the data. The requisite E-index data are from http://www.law.harvard.edu/faculty/bebchuk/data.shtml which provides coverage up to 2006.

[7] *General ability* is the first factor of the principal components analysis based on five measures. These measures capture prior work experience for a given CEO before her current CEO position. The five aspects captured include the number of unique positions, the number of unique firms, and the number of unique industries in which a given CEO worked in the past plus whether she held a CEO position prior to her current CEO position and whether she worked for a multi-divisional firm prior to her current CEO position. The requisite data are from https://sites.google.com/site/claudiapcustodio/research, which provides coverage for CEOs of S&P 1500 firms from 1993 through 2007. We thank Professor Custodio for providing the data.

[8] Our results are also robust to adding CRSP closing bid-ask spread to isolate the effect of stock market liquidity (Chung and Zhang, 2014) on executive compensation (e.g., Jayaraman and Milbourn, 2011). These results are not tabulated.

We use Cooperative Congressional Election Study (CCES) data on general election voter turnout to construct an alternate measure for social capital. The variable of choice is *vv_turnout_gvm*, which contains self-reported data of voting behavior for each respondent (voted = 1, did not vote = 0). This variable could capture the strength of civic norms in the local area and reflect the influences arising from social capital. The requisite data are from http://cces.gov.harvard.edu/pages/welcome-cooperative-congressional-election-study. CCES surveys were conducted in 2006, 2008, 2010, 2012, and 2014 during our sample period. For observations before 2006, we use CCES 2006 data to backfill missing observations. Specifically, we compute the variable, *CCES self-reported voter turnout*, using the county-level average of the *vv_turnout_gvm* variable; and, we re-estimate the regression models after replacing *Social capital* with *CCES self-reported voter turnout*. The first two columns of Table 3, Panel D, report the results from this analysis. The coefficients on *CCES self-reported voter turnout* are negative and significant with *p*-values equal to 0.06 or better, indicating that our results are robust to using the CCES voter turnout measure as an alternate proxy for social capital.[9]

Because of data limitations in NRCRD, we construct *Social capital* using the backfilling method. This method could overstate the significance of the estimates. We perform two analyses to ease this concern. First, as do Alesina and La Ferrara (2000) and Hilary and Hui (2009), we estimate the regressions using linearly interpolated social capital data that involve generating the values in the missing years by linear approximation. Second, we perform regressions using only

[9] Our results are also robust to another alternative measure of social capital using county-level blood donation data from the DDB Life Style Survey, which covers the period 1992–1994. Specifically, we re-estimate the baseline models after replacing *Social capital* with the dummy variable, *High blood donation*, which equals one if a county's average blood donation rate in 1992–1994 ranks in the top quartile and equals zero otherwise. These results are not tabulated.

the three years in which data on social capital are actually available in the NRCRD, namely, 1997, 2005, and 2009. The reduced sample contains 3,851 firm-year observations. Table 3, Panel D, reports the results from the baseline regression model based on these approaches. Across the models, the coefficients on *Linearly interpolated social capital* and *Social capital* are negative and generally significant at better than the 1% level, indicating that the backfilling method does not excessively overstate the significance of the baseline regression estimates.

We use county-level clustered standard errors in our estimations to ease the concern that correlation of CEO pay practices among firms co-located in the same county might overstate the significance of the estimates in the baseline regressions. We provide evidence to further mitigate this concern by analyzing data at the county-year level. Specifically, we calculate the mean values of all firm-level variables based on firms located in the same county in a given year. We estimate the regressions using these county-level variables in place of the corresponding firm-level variables. We drop industry dummies from this revised regression specification because it is not meaningful to use average values based on industry dummies. Table 3, Panel E, reports results from these regressions. The sample in this analysis contains 390 unique counties with 5,549 county-year observations. Across the models, the coefficients on *Social capital* remain negative and significant at the 1% level.

4.5. Regression results using inherited cultural preferences as instruments for social capital

Reverse causality could make it challenging to infer a causal relation between social capital and CEO pay. However, culture is likely transmitted across generations, and individuals are likely influenced by cultural preferences that they inherit from their ancestral origins (e.g., Becker, 1996; Guiso, Sapienza, and Zingales, 2006). Accordingly, we use an epidemiological approach (e.g., Guiso Sapienza, and Zingales, 2004; Algan and Cahuc, 2010; Fernández, 2011; Luttmer and Singhal, 2011) to construct instruments for social capital based on cultural preferences from people's respective countries of ancestry, namely, inherited cultural preferences. This approach is advantageous because the corresponding instruments are likely unaffected by reverse causality.

There is substantial evidence of cultural persistence: that the parent's attitudes, values, and behaviors are good predictors of the attitudes, values, and behaviors of children (e.g., Fernández, Fogli, and Olivetti, 2004; Fernández and Fogli, 2009; Algan and Cahuc, 2010). Based on this logic, we use ancestry data from the Census Bureau and two specific Hofstede's scores of national culture, namely, power distance and masculinity-femininity, to construct our instruments for social capital. The Hofstede's score data are mainly from https://harzing.com/download/hgindices.xls, which we supplement with additional data from https://www.hofstede-insights.com. Census ancestry data report the first ancestry of people residing in each county (Fernández, 2007), which we use to calculate the percentages of peoples' countries of ancestry within a county. We then construct the instrumental variable, *Power distance* (*Masculinity-femininity*), using a weighted average method that combines these percentages with the Hofstede's scores for power distance (masculinity-femininity) based on people's respective countries of ancestry.

Putnam (2001) observes that US states with greater tolerance for equality have higher social capital, concluding that social capital and tolerance for equality "go together." Accordingly, we expect that power distance is negatively associated with social capital because power distance reflects an attitude toward greater tolerance for inequality among people (Hofstede, 2003). Masculinity-femininity measures the relative strength of masculine social values against feminine social values, where masculine values emphasize the importance of material success and feminine values emphasize the importance of building relationships with people and helping others (Hofstede, 2003). Given that "social capital exists in the relation among persons" (Coleman, 1988, p. S100), our conjecture is that masculinity-femininity is negatively associated with social capital.

In our setting, a valid instrument should correlate with social capital. We find evidence that our instruments satisfy this requirement. Model 1 in Table 4 reports the results. In this regression, the dependent variable is *Social capital* and independent variables include *Power distance*, *Masculinity-femininity*, and all control variables as specified in the baseline model. The coefficients on *Power distance* and *Masculinity-femininity* are negative and significant at the 1% level. Moreover, the Angrist-Pischke (2009) *F*-statistic for weak instruments is significant at the 1% level, suggesting that the instruments are not weak.

[Insert Table 4]

Models 2 and 3 of Table 4 report the second-stage regression results. We continue to use the baseline models for these regressions, except that we replace *Social capital* with *Fitted social capital*, where the latter is generated from estimates in the first-stage regression. Across the models, the coefficients on *Fitted social capital* remain negative and retain their significance at either the 1% level or the 10% level (p-value = 0.08). Given the epidemiological design, these findings offer plausible causal evidence of social capital's effect on CEO compensation.

4.6. Results from propensity score matched sample

Nevertheless, if corporate headquarter location decision is endogenous, social capital could be endogenous too. We use the propensity score matched technique to mitigate this concern. Caliendo and Kopeinig (2008) provide a useful survey of this method. In our case, the idea is to compare CEO compensation in a treatment group against a control group, where treatments are firms located in a county with a high level of social capital and controls are firms with comparable propensity of locating in a high-social-capital county based on firm fundamentals but are actually located in a county with a lower level of social capital.

Specifically, from 1993 to 2014, we rank *Social capital* annually based on the data in that year; and, we classify those firm-years in the top quartile as treatment and those in the bottom quartile as control. For each treatment firm-year, the dummy variable, *High social capital*, equals one; for each control firm-year, *High social capital* equals zero. This procedure generates 5,562 firm-years in the treatment group and a similar number of observations in the control group. Relying on this sample, we generate the propensity score by running a logistic regression with *High social capital* as the dependent variable; the independent variables include all variables as specified in the baseline model. We then match, without replacement, each treatment observation (*High social capital* = 1) with a unique control (*High social capital* = 0) using the closest propensity score. We use a caliper of 1% to find the closest match, where caliper refers to the difference in the predicted propensity scores between the treatment and match. Based on these procedures, we identify 787 matched pairs of treatment-control observations.

Table 5, Panel A, presents results of the Student's *t*-tests that compare firm and county attributes across treatment sample (*High social capital* = 1) and control sample (*High social capital* = 0). As expected, there is a significant difference in *Social capital*. The other results show no significant difference in any variable across the two samples, except for *Population density* which is significant at the 10% level.

[Insert Table 5 here]

We explore the effect of the treatment against the counterfactual using the test sample that includes 1,574 firm-year observations, of which 787 firm-years are from the treatment firms and 787 are from the control firms. As before, we use the baseline regression models; in this case, we modify the model by replacing *Social capital* with *High social capital*. This specification produces estimates on *High social capital* that capture the average treatment effects by comparing observations in firms headquartered in high-social-capital counties against observations in firms with comparable propensity scores but actually have headquarters located in counties with lower levels of social capital, holding other factors constant. Table 5, Panel B, reports the results. Across the models, estimates on *High social capital* remain positive and significant at better than the 10% level.

4.7. Corroborating evidence based on headquarter relocations

Firms seldom relocate corporate headquarters and over-time variations in social capital dwarf cross-sectional variations in social capital in our data. This raises the concern that our results are primarily driven by cross-sectional variations in social capital. We provide evidence to allay this concern by analyzing the effects of over-time variations in social capital, focusing on those firms that relocated corporate headquarters to another county with a different level of social capital. Using a difference-in-differences method, we explore how over-time changes in social capital affect over-time changes in CEO compensation across firms that either experienced a social-capital-increasing relocation or a social-capital-decreasing relocation.

Based on the sample of 2,396 unique firms in the main analysis, we identify 76 firms with either one social-capital-increasing relocation or one social-capital-decreasing relocation in the period 1996–2010 with at least three years of data before and three years of data after the relocation.

Of these 76 firms, there are 35 firms with a social-capital-increasing relocation event and 41 with a social-capital-decreasing relocation event, providing a total of 959 firm-years for the analysis. Of these observations, 421 firm-years are from the before-relocation period and 538 are from the after-relocation period.

We re-estimate the baseline models after replacing *Social capital* with three variables, namely, *Social-capital-increasing relocation*, *After*, and the interaction variable, *After* × *Social-capital-increasing relocation*. *Social-capital-increasing relocation* is a dummy variable that equals one or zero depending on whether the firm relocated its headquarter to a county with a higher or lower level of social capital. *After* equals one or zero depending on whether the firm-year observation is from the period after or before the relocation.

We are particularly keen on the coefficient of the interaction term because it provides an estimate of the difference in over-time changes in CEO compensation between firms that experienced a social-capital-increasing relocation and firms that experienced a social-capital-decreasing relocation across the two periods surrounding the relocation events. Table 6, Panel A, reports the results. Across the models, the coefficients on the interaction terms are negative and significant; it is significant at the 5% level for the *Total pay* regression (p-value = 0.03) and 10% level for the *Equity pay* regression (p-value = 0.09). These results show that changes in social capital over time can explain temporal changes in CEO compensation. More specifically, firms with a social-capital-increasing relocation display significantly larger temporal reduction in CEO compensation when compared to firms with a social-capital-decreasing relocation.

[Insert Table 6]

We perform two tests to provide credence that the documented results are attributable to changes in social capital resulting from the relocation decisions. First, we test whether firm attributes in the subsample that experienced a social-capital-increasing relocation are comparable to those in the subsample that experienced a social-capital-decreasing relocation. In particular, we use the Student's *t*-test to formally test whether firm attributes and CEO pay levels are different across these two groups of firms in the year immediately before relocation. Panel B of Table 6 reports the results. The findings reveal no significant differences between the two groups of firms in any of the dimensions examined, except *CEO age* (*p*-value = 0.08). Second, we test whether changes in corporate strategies surrounding relocations are comparable across the two subsamples. If headquarter relocations are motivated by changing business conditions facing firms, one would expect to observe changes in corporate strategies surrounding relocation events. We capture strategies such as growth (market-to-book and firm size), capital structure (leverage), risk undertaking (return volatility), and diversification (number of segments). We find that the changes in these variables (i.e., market-to-book, firm size, leverage, return volatility, and number of segments) surrounding relocation events are not significantly different across the two groups of firms. These results are not tabulated.

5. Social capital and the power-pay relation

So far, our results show a robust relation between social capital and CEO compensation, providing tentative evidence that social capital restrains managerial rent extraction in CEO compensation. In this section and the following section, we conduct additional analyses to pin down this inference. First, we follow Adams, Almeida, and Ferreira (2005) to examine how social capital affects the positive relation between CEO power and CEO pay. Second, we follow Narayanan and Seyhun (2008), Bebchuk, Grinstein, and Peyer (2010), and Lie (2005) to examine the effects of social capital on incidences of CEO option grant awards that likely embody consequences of managerial rent extraction, including unscheduled awards, lucky awards, and backdated awards.

Adams, Almeida, and Ferreira (2005) find that, on average, powerful CEOs extract higher compensation. Morse, Nanda, and Seru (2011) find that powerful CEOs weaken the effectiveness of incentive by rigging the weights used in these arrangements toward performance measures that are doing better. These findings are consistent with the managerial power approach put forth by Bebchuk, Fried, and Walker (2002), which implies a positive power-pay relation, namely, a positive relation between CEO power and levels of CEO pay. Our hypothesis maintains, and the results so far show that social capital is negatively associated with the levels of CEO pay. If these findings were to provide evidence of social capital's disciplinary effect on managerial rent extraction in the CEO compensation setting, one would naturally predict that social capital weakens the accretive effect of CEO power on CEO pay. We examine this implication and provide corroborating evidence as follows.

Following Adams, Almeida, and Ferreira (2005) and Morse, Nanda, and Seru (2011), we use a CEO power measure that captures the CEO's personal influence over the board of directors. The dummy variable, *CEO power*, equals one if a firm's CEO also serves as the chairperson of the board and president of the company in a given year; *CEO power* equals zero otherwise. We modify the baseline model in two ways. First, we replace *Social capital* with a dummy variable, *High SK*, which specifically captures those firms headquartered in counties with higher levels of social capital. *High SK* equals one if *Social capital* for a firm in a given year is higher than the sample median; it equals zero otherwise. Second, we add *CEO power* and the interaction term, *CEO power* × *High SK*, to the baseline model. In this specification, a negative coefficient on the interaction term is consistent with the notion that social capital weakens the accretive effect of CEO power on CEO compensation.

Models 1 and 2 in Panel A of Table 7 present estimates of the revised model. We also present the estimates of two alternate models, in Models 3 and 4, in which we drop the interaction term from the revised model. Across the models, the coefficients on *CEO power* are positive and significant, confirming that a positive power-pay relation exists in our data. More importantly, we find that the estimates on the interaction term in Models 1 and 2 are negative, as expected, and they are significant at the 5% and 10% levels, respectively.

[Insert Table 7]

Coles, Daniel, and Naveen (2014) and Khanna, Kim, and Lu (2015) find that CEOs could exert greater personal influence over those board members who are appointed after the CEO assumed the office of CEO; hereafter, we refer to these board members as CEO appointed directors. Accordingly, we use an alternate measure of CEO power that captures the proportion of CEO appointed directors on a board in a given year. We obtain the requisite data on CEO appointed directors from Professor Lalitha Naveen; this sample is derived from the sample analyzed by Coles, Daniel, and Naveen (2014). Based on these data, we construct the variable, *% CEO appointed directors*, which is the number of CEO appointed directors divided by the total number of board members for a firm in a given year. After merging these data into our sample, we obtain a final sample of 14,587 firm-year observations for the analysis.

We run the same regressions as those employed in Panel A of Table 7 after replacing *CEO power* with *% CEO appointed directors*. Table 7, Panel B, reports these results. Across the models, the coefficients on *% CEO appointed directors* are positive and significant at the 1% level. Again, the estimates on the interaction term in Models 1 and 2 are negative, as expected, and they are significant at the 5% level or better.

6. Social capital and opportunistic timing in CEO option grant awards

Equity-based compensation typically constitutes a large portion of the CEO's total pay; and there is significant evidence that CEO option grant awards are especially vulnerable to managerial rent extraction (e.g., Yermack, 1997; Bebchuk, Fried, and Walker, 2002). More importantly, prior research identifies specific equity-based awards, such as unscheduled awards, lucky awards, and backdated awards, as opportunistic timing practices in CEO option grant awards that unduly favor CEOs. Narayanan and Seyhun (2008) find that firms can afford their CEOs with substantial latitude to "designate option grant dates to increase their compensation" through the use of unscheduled awards. Lie (2005) and Heron and Lie (2009) find that stock returns are generally negative before option grants but they are generally positive after option grants, suggesting that option awards could be backdated. Bebchuk, Grinstein, and Peyer (2010) find evidence that CEOs receive an abnormally high number of lucky grants; and, those CEOs who receive lucky grants also tend to have higher total compensation, even after controlling for other factors affecting compensation. Based on these findings, we use incidences of unscheduled awards, incidences of lucky awards, and incidences of backdated awards to capture exemplifications of various consequences of managerial rent extraction in CEO equity-based compensation practices.

Accordingly, we examine the extent to which social capital affects the likelihood that firms make each of these awards to their CEOs.

6.1. Measures of opportunistic timing

A commonality of the aforementioned studies is the focus on at-the-money, unscheduled option grants awarded to CEOs. This focus is warranted because unscheduled grants are more susceptible to managerial influences when compared to awards that follow a roughly fixed timing pattern (e.g., Narayanan and Seyhun, 2008; Heron and Lie, 2009; Bebchuk, Grinstein, and Peyer, 2010). Accordingly, we focus all ensuing analyses on unscheduled option grant awards to CEOs. We define a scheduled award as an option grant award with the same grant date plus/minus one day of another grant awarded to the same CEO in the preceding year. We define all other awards as unscheduled.

We use three measures to capture the consequences of opportunistic timing in CEO option grant awards. Because we conduct the main analysis at the firm-year level, we construct these measures at firm-year level as well. The variable, *Unscheduled*, equals one if a firm in a given year makes at least one unscheduled award to its CEO; otherwise, it equals zero. The variable, *Backdated*, equals one if the mean cumulative abnormal returns (CAR) of all unscheduled awards that a firm grants its CEO in a given year is at the top 30th percentile of CAR in the sample; otherwise, it equals zero. For a given award, we calculate CAR as the difference in cumulative daily stock price returns over two five-day windows, (-5, -1) and (+1, +5), straddling the date of the unscheduled award (day 0). The variable, *Luck*, equals one if a firm in a given year makes at least one unscheduled award to its CEO on a date that has the lowest price in the grant month.

We collect observations of option grant awards from the Thomson Financial Insider Trading database, which covers all insiders' filings of equity transactions in Forms 3, 4, 5, and 144. We treat multiple awards to the same individual on the same day as one observation. An award is an event on a given date in which the CEO in a given company receives one or more option grants, where a CEO is an individual identified in the Thomson database as either the CEO or the President of the company (rolecode equal to CEO or P). The sample covers the time period 1993–2006. This is reasonable because the Securities and Exchange Commission issued revised rules that mandate new detailed disclosures of executive compensation practices in 2006, and prior studies examining opportunistic CEO option awards typically examine observations prior to 2006 (e.g., Heron and Lie, 2009; Bebchuk, Grinstein, and Peyer, 2010).

6.2. Effects of social capital on incidences of unscheduled awards

We are able to identify 11,149 firm-years with at least one unscheduled award in a given year (*Unscheduled* = 1) and 1,663 firm-years with no unscheduled award in a given year (*Unscheduled* = 0). We use this sample containing 12,812 (= 11,149 + 1,663) firm-years to examine the effect of social capital on the likelihood that a firm makes at least one unscheduled award to its CEO in a given year. We estimate coefficients of the baseline model using Probit regressions after replacing the dependent variable with the dummy variable, *Unscheduled*. Model 1, Table 8, reports the results. The sample contains 8,508 firm-year observations. Sample attrition is due to missing observations in the two variables that capture CEO characteristics, namely, *CEO tenure* and *CEO age*.[10] Model 2 presents the results from an alternate model that excludes these

[10] To preserve observations, in the ensuing analyses, we use CEO age and CEO tenure information from the BoardEx database to supplement corresponding missing data in the ExecuComp database.

two variables; it uses the full sample of 12,812 firm-year observations. Across the models, the coefficients on *Social capital* are negative and significant at the 5% level or better. Based on coefficients in Model 2, the implied probability that a firm makes at least one unscheduled award to its CEO in a given year is 90.50% when *Social capital* is at the 25th percentile and other independent variables are evaluated at their respective mean values in the sample. The corresponding implied probability is 88.11% at the 75th percentile of *Social capital.* Accordingly, an interquartile increase in *Social capital* in the data reduces the likelihood of an unscheduled award by about 2.39% (2.39%=90.50% - 88.11%). By way of comparison, the corresponding unconditional likelihood in the sample is around 87.02% (87.02%=11,149 ÷ 12,812).

[Insert Table 8]

6.3. Effects of social capital on option backdating

As do Lie (2005) and Heron and Lie (2009), we focus on option backdating in unscheduled awards only. Using the 11,149 firm-years with at least one unscheduled award, we identify 3,207 firm-years in which firms backdated CEO option awards (*Backdated* = 1) and 7,942 firm-years with CEO awards that are not backdated (*Backdated* = 0).

We estimate the baseline regression model using Probit regressions after replacing the dependent variable with the dummy variable *Backdated*. Table 9, Model 1, reports the results. Model 2 presents the results using an alternate model based on the same revised specification after excluding the *CEO tenure* and *CEO age* variables from the model. Across these models, the coefficients on *Social capital* are negative and significant with *p*-values equal to 0.03 and 0.07, respectively. Based on the coefficients in Model 2, an interquartile increase in *Social capital* from the 25th percentile to the 75th percentile in the data would reduce the likelihood that a firm makes a backdated award to its CEO in a given year by about 1.89% (1.89%=24.85% - 22.96%) when other independent variables are evaluated at their respective mean values in the sample. This effect is sizable and economically meaningful as the unconditional likelihood of backdated awards in the sample is 29% (29%=3,207 ÷ 11,149).

[Insert Table 9]

6.4. Effect of social capital on incidences of lucky awards

We obtain firm-year level information to determine luck in CEO option awards from Professor Lucian Bebchuk. The data reflect the subset of the sample analyzed by Bebchuk, Grinstein, and Peyer (2010) that overlaps with our main sample. It contains 9,872 firm-years between 1996 and 2005 of which around 14% include at least one lucky CEO award; that is, the firm in that year is identified as giving at least one lucky award to the CEO (*Luck* = 1). As before, we replace the dependent variable of the baseline model with the variable of interest and re-estimate the revised model. In this case, the variable of interest is the dummy variable, *Luck*. Accordingly, we use Probit regressions to estimate the model. Model 1, Table 10, reports the results. Model 2 presents the results from an alternate model, which excludes *CEO tenure* and *CEO age*. Across the models, the coefficients on *Social capital* are negative and significant with *p*-values equal to 0.06 and 0.04, respectively. Based on the coefficients in Model 1, an interquartile increase in *Social capital* from the 25^{th} percentile to the 75^{th} percentile in the data would reduce the likelihood that a firm makes a lucky award to its CEO in a given year by about 2.52%. (2.52%=12.01% - 9.49%) when other independent variables are evaluated at their respective mean values in the sample. The implied reduction is sizable and economically meaningful as the unconditional likelihood of lucky awards in the sample is 14% in the entire sample.

[Insert Table 10]

7. Conclusion

This study provides a novel analysis of the impact of social capital on one specific form of agency problem, namely, managerial rent extraction in CEO compensation. We capture broad consequences of managerial opportunism using the levels of CEO total pay and CEO equity-based pay. We capture opportunistic timing in CEO option awards using incidences of lucky awards, backdated awards, and unscheduled awards; these measures embody more specific forms of managerial opportunism in CEO compensation setting. The results indicate that firms headquartered in US counties with higher levels of social capital, as captured by strong cooperative norms and dense social networks, are less likely to make lucky awards, backdated awards, and unscheduled awards to their CEOs. And, the levels of CEO pay, both in total pay and in equity-based pay, are lower in these firms even after controlling for other factors that are known to affect CEO compensation. We interpret these findings as indicating that social capital deters managerial rent extraction in CEO pay. To fortify this interpretation, we provide corroborating evidence that a positive power-pay relation exists in our data and, more importantly, that social capital moderates this positive relation. Our social capital construct is specifically designed to capture secular, social influences arising from norms and networks in localities surrounding corporate headquarters. As such, the findings point to an important insight: that secular, social influences could constrain agency problems in corporations.

The implications are quite broad. First, agency problems are pervasive in publicly listed corporations. If social capital constrains agency problems in CEO pay, it could also matter in other settings where agency problems are significant. Second, there is emerging evidence that social capital matters in corporate decisions (e.g., Hasan, Hoi, Wu, and Zhang, 2017a; Hoi, Wu, Zhang, 2017), in debt contracting (Hasan, Hoi, Wu, and Zhang, 2017b), and even in audit pricing (Jha and Chen, 2015). Taken together, these findings indicate that it is fruitful to explore the impact of social capital and, more broadly, other social factors in the corporate setting.

Appendix

Variable definition and measurement

This appendix presents definitions of the variables in the baseline regression model. Data for executive compensation are from Standard and Poor's Compustat ExecuComp database. Data for firm attributes are from Standard and Poor's Compustat database and CRSP database. Data for institutional ownership are from Thomson Reuters Ownership database. Social capital data are from the Northeast Regional Center for Rural Development (NRCRD) at the Pennsylvania State University. The county-level demographic data are from the Bureau of Economic Analysis, the US Census Bureau, and the Association of Religion Data Archives (ARDA).

Variable	Definition
Measures of executive compensation in year *t*+1 (dependent variables for baseline regressions):	
Total pay	Logarithm of one plus total compensation for a CEO (CEO total pay as reported in ExecuComp) during a given year.
Equity pay	Logarithm of one plus equity-based compensation for a CEO (CEO equity pay as reported in ExecuComp) in a given year, including restricted stocks and option grants.
Salary	Logarithm of one plus salary received by a CEO (CEO cash salary as reported in ExecuComp) in a given year.
Bonus	Logarithm of one plus cash bonus received by a CEO (CEO cash bonus as reported in ExecuComp) in a given year.
Other pay	Logarithm of one plus the sum of long-term performance payout in incentive plans (before 2006), deferred compensation (after 2006), contribution to retirement plan, perquisites, change-in-control payments, other personal benefits, etc. (CEO other pay as reported in ExecuComp) in a given year.
Measure of social capital in year *t* (Test variable for baseline regressions):	
Social capital	First principal component from a factor analysis based on *Pvote*, *Respn*, *Nccs*, and *Assn* data from NRCRD. *Pvote* reports voter turnouts in presidential elections. *Respn* reports response rates in US census surveys. *Assn* is the total numbers of ten types of social organizations. *Nccs* is the total numbers of nonprofit organizations.
Control variables in year *t*:	
CEO age	Age of the CEO.
CEO tenure	Number of years since a CEO has been a CEO for the firm.
IO total	Fraction of a firm's shares held by institutional investors.
IO concentration	Sum of the squares of the fraction of shares held by each institutional investor.
Firm size	Logarithm of total sales for a firm in a year.
Return	Average monthly return for a stock in a year.
Return volatility	Standard deviation of monthly returns for a stock in a year.
ROA	Return on assets for a firm in a given year.

M/B	Ratio of market value of assets scaled by book value of assets.
Leverage	Ratio of total liabilities (DLC + DLTT) scaled by total assets.
Cash	Ratio of cash and marketable securities divided by total assets.
CAPEX	Ratio of capital expenditure divided by total assets.
County income	Median per capita household income (in thousands $) in a county during a year.
County age	Median age of the residents in a county during a year.
Education	Percentage of people 25 years old and above with at least one year of college in a county in a given year, times 100.
Religiosity	Fraction of a county's residents that claim affiliation with an organized religion in a given year as reported in the Association of Religion Data Archives (ARDA) data.
Political strength	Relative strength of Democratic/Republican party as manifested in electoral outcomes in states where the firm is headquartered. See Rubin (2008) and Hoi, Wu, and Zhang (2013) for more information. The electoral outcome data are obtained from http://thegreenpapers.com.
Population growth	Rate of change in a county's population for the past year.
Nonwhite population	One minus the fraction of a county's residents reported as white in US Census.
Population density	Ratio of the number of residents (in 1,000s) per square mile of land area in the county.
Latitude	Latitude of the county in which a firm's headquarter is located.
Longitude	Longitude of the county in which a firm's headquarter is located.
Distance to river	Closest distance between a firm's headquarter location and the main stream of the ten longest rivers in the US, including Missouri River, Mississippi River, Yukon River, Rio Grande, Colorado River, Arkansas River, Columbia River, Red River, Snake River, and Ohio River.
CEO power	An indicator that equals one if a firm's CEO also serves as the chairperson of the board and president of the company in a given year, and equals zero otherwise.
% CEO appointed director	The number of directors appointed after the CEO assumed office divided by the total number of directors on the board for a firm in a year.

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Table 1
Summary statistics

The sample contains 22,246 firm-year observations during 1993–2014. Compensation data are from the ExecuComp database. *Total pay* is the natural logarithm of one plus total compensation for a CEO during a given year (*CEO total pay*) as reported in ExecuComp. *Equity pay* is the natural logarithm of one plus equity-based compensation for a CEO in a given year (*CEO equity pay*) as reported in ExecuComp, which mainly consists of restricted stocks and option grants. *Social capital* is an estimate of a county's social capital index based on data from the Northeast Regional Center for Rural Development (NRCRD) at the Pennsylvania State University. Other variables are defined in the Appendix.

Variables	N	Mean	S.D.	25th	Median	75th
Total pay	22,246	7.9396	1.0264	7.2079	7.9580	8.6810
Equity pay	22,246	6.2304	2.8961	5.7688	7.1414	8.1312
Salary	22,246	6.4462	0.4955	6.1373	6.4881	6.8163
Bonus	22,246	3.0983	3.1663	0.00	2.9246	6.2166
Other pay	22,246	4.9266	2.5190	3.0350	5.3210	6.9955
Social capital	22,246	-0.4408	0.8339	-1.1273	-0.3892	0.1677
CEO age	22,246	55.57	7.34	51.00	56.00	60.00
CEO tenure	22,246	7.4028	7.44	2.0000	5.0000	10.00
IO concentration	22,246	0.0283	0.0368	0.0142	0.0233	0.0347
IO total	22,246	0.7051	0.2170	0.5676	0.7374	0.8758
Firm size	22,246	7.1459	1.5327	6.1384	7.1140	8.2426
Return	22,246	0.0137	0.0359	-0.0048	0.0133	0.0313
Return volatility	22,246	0.1110	0.0658	0.0662	0.0946	0.1361
ROA	22,246	0.0427	0.1038	0.0156	0.0490	0.0905
M/B	22,246	1.9653	1.2886	1.1660	1.5233	2.2295
Leverage	22,246	0.2234	0.1837	0.0579	0.2059	0.3426
Cash	22,246	0.1465	0.1733	0.0219	0.0748	0.2099
CAPEX	22,246	0.0534	0.0529	0.0188	0.0386	0.0700
County income	22,246	42.4760	16.1107	31.6130	39.4710	49.2600
County age	22,246	34.3856	2.3209	32.00	34.00	36.00
Education	22,246	32.3009	10.2392	25.20	30.80	40.30
Religiosity	22,246	0.5412	0.1182	0.4413	0.5434	0.6316
Political strength	22,246	3.3235	75.6292	-67.00	50.00	75.00
Population growth	22,246	0.0094	0.0105	0.0026	0.0076	0.0141
Nonwhite population	22,246	0.2248	0.1214	0.1353	0.2165	0.3151
Population density	22,246	3.9517	10.3129	0.6531	1.2916	2.1017
Latitude	22,246	38.5682	4.4860	35.1228	39.9642	41.7051
Longitude	22,246	-91.4769	17.0384	-97.5601	-86.8210	-76.6777
Distance to river	22,246	213.80	159.71	86.10	193.60	317.20
CEO power	22,246	0.2655	0.4416	0.0000	0.0000	1.00
% CEO appointed directors	14,587	0.4701	0.3186	0.2000	0.4286	0.7272
Summary statistics of CEO compensation data from ExecuComp						
CEO total pay (in $1,000)	22,246	4605.92	4946.17	1349.05	2857.45	5889.05
CEO equity pay (in $1,000)	22,246	2612.24	3543.93	319.15	1262.23	3398.02
CEO cash salary (in $1,000)	22,246	704.01	319.44	461.78	656.25	911.63
CEO cash bonus (in $1,000)	22,246	398.22	712.58	0.00	17.63	500.00
CEO other pay (in $1,000)	22,246	944.55	1815.55	19.80	203.59	1090.67

Table 2
Baseline regressions

The sample contains 22,246 firm-year observations during 1993–2014. Regression models are as specified in the baseline models in Eq. (1). *Total pay* (*Equity pay*) is the logarithm of one plus total compensation (equity-based compensation) for a CEO during a given year as reported in the ExecuComp database. *Social capital* is a county-level social capital index based on data from the NRCRD at the Pennsylvania State University. Other variables, including other compensation variables (i.e., *Salary, Bonus,* and *Other pay*), are defined in the Appendix. In Panel B, control variables include all firm-level and county-level variables plus industry and year fixed effects as reported in Panel A. Estimates are based on ordinary least squares regressions with standard errors adjusted for heteroskedasticity and within-county clustering. *t*-statistics, in parentheses, are based on two-sided tests. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

Panel A: Social capital and total/equity CEO pay

Variables	*Total pay*	*Equity pay*
Social capital	-0.0637***	-0.1468***
	(-4.25)	(-2.69)
CEO age	-0.0002	-0.0150***
	(-0.15)	(-3.06)
CEO tenure	-0.0060***	-0.0407***
	(-3.25)	(-7.59)
IO concentration	-1.7761***	-5.8941***
	(-4.38)	(-5.84)
IO total	0.7024***	2.3264***
	(9.82)	(11.07)
Firm size	0.4266***	0.5709***
	(34.85)	(16.04)
Return	2.4469***	2.8610***
	(12.65)	(4.01)
Return volatility	-0.3223*	-0.7415
	(-1.89)	(-1.28)
ROA	-0.5934***	-1.3942***
	(-4.79)	(-4.37)
M/B	0.0988***	0.1726***
	(8.90)	(4.27)
Leverage	0.1902***	-0.0054
	(3.17)	(-0.03)
Cash	0.5045***	0.4115
	(5.48)	(1.42)
CAPEX	-0.1461	0.7755
	(-0.65)	(1.29)
County income	0.0024	0.0015
	(1.11)	(0.18)
County age	0.0102	0.0319
	(1.50)	(1.43)
Education	0.0042*	0.0086
	(1.94)	(1.03)
Religiosity	0.1370	0.1240
	(1.26)	(0.37)
Political strength	0.0001	-0.0005
	(0.42)	(-0.83)
Population growth	-2.4523**	-3.8630
	(-2.18)	(-1.05)
Nonwhite population	0.2385**	0.5094*

	(2.48)	(1.92)
Population density	-0.0004	-0.0046
	(-0.29)	(-0.75)
Latitude	-0.0051	0.0019
	(-1.31)	(0.16)
Longitude	0.0002	-0.0017
	(0.26)	(-0.60)
Distance to river	0.0001	0.0002
	(0.93)	(0.68)
Industry and year dummies	Yes	Yes
Adjusted R^2	0.5333	0.2307
Number of observations	22,246	22,246

Panel B: Social capital and other components of CEO pay

Variables	*Salary*	*Bonus*	*Other pay*
Social capital	-0.0061	-0.1011**	0.0317
	(-0.53)	(-1.97)	(0.68)
All control variables	Yes	Yes	Yes
Adjusted R^2	0.5630	0.3950	0.3321
Number of observations	22,246	22,246	22,246

Table 3

Omitted variables and other sensitivity analyses

The sample contains 22,246 firm-year observations during 1993–2014. *Total pay* (*Equity pay*) is the logarithm of one plus total compensation (equity-based compensation) for a CEO during a given year as reported in the ExecuComp database. Regressions are based on the baseline models of Eq. (1) with modifications as specified below. Panel A presents estimates of regressions after adding omitted variables for preferences for redistribution, income inequality, and metropolitan setting. *County redistribution preferences* is the mean of ANES respondent data for question VCF0809 for a given county across all years in which ANES conducted a survey in the county. We code the data such that a higher number means that a respondent is more favorable to redistribution. *Income inequality* is Gini coefficients for US counties from 2006 to 2014 as reported in the American Community Survey (Variable B19083). Data for the missing years before 2006 are filled in using year 2006 data. *Metro* equals one if a firm's headquarter is located within a 250-kilometer radius of a metropolitan statistical area with more than one million residents according to the 2010 census; *Metro* equals zero otherwise. Panel B presents estimates of regressions with region fixed effects and state fixed effects, where the regions as defined by the Census Bureau, including New England, Mid-Atlantic, East North Central, West North Central, South Atlantic, East South Central, West South Central, Mountain, and Pacific areas. Panel C presents estimates of regressions with added county-level, firm-level, and executive-level controls. *Other total pay* (*Other equity pay*) is the mean of *Total pay* (*Equity pay*) for other firms headquartered in the same county in a given year. *E-Index* is the total number of anti-takeover provisions a firm has in a given year, including staggered boards, limits to shareholder bylaw amendments, poison pills, golden parachutes, and supermajority requirements for mergers and charter amendments (Bebchuk, Cohen, and Ferrell, 2009). *General ability* is the first factor of the principal components analysis of five proxies of a given CEO's general managerial skills (Custodio, Ferreira, and Matos, 2013). These proxies include past number of (1) positions, (2) firms, and (3) industries in which a CEO worked; (4) whether the CEO held a CEO position at a different company; and (5) whether the CEO worked for a conglomerate. Panel D presents regression results based on alternative social capital measures and alternate sampling methods. *CCES self-reported voter turnout* is the county-level average of the *vv_turnout_gvm* variable provided by the Cooperative Congressional Election Study (CCES), which contains self-reported voting turnout data for each respondent (voted = 1, did not vote = 0). CCES conducted surveys in 2006, 2008, 2010, 2012, and 2014. We backfill data for the missing years using CCES self-reported voter turnout measure in the preceding year in which data are available and we backfill missing observations before 2006 using CCES data in 2006. *Linearly interpolated social capital* uses a linear interpolation method to generate data for social capital in missing years. The last two columns of Panel D present regressions using a reduced sample for which data on social capital are actually available in the NRCRD, i.e., 1997, 2005, and 2009. Panel E reports regressions using the county-level average values of the variables. Across all panels and in all regressions, control variables include *CEO age*, *CEO tenure*, *IO concentration*, *IO total*, *Firm size*, *Return*, *Return volatility*, *ROA*, *M/B*, *Leverage*, *Cash*, *CAPEX*, *County income*, *County age*, *Education*, *Religiosity*, *Political strength*, *Population growth*, *Nonwhite population*, *Population density*, *Latitude*, *Longitude*, and *Distance to river* plus industry and year fixed effects as specified in the baseline models. All of these variables are defined in the Appendix. Estimates are based on ordinary least squares regressions with standard errors adjusted for heteroskedasticity and within-county clustering. *t*-statistics, in parentheses, are based on two-sided tests. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

Variables	*Total pay*	*Equity pay*	*Total pay*	*Equity pay*	*Total pay*	*Equity pay*
Panel A: Effects of preferences for redistribution, income inequality, and metropolitan setting						
Social capital	-0.0529***	-0.1669***	-0.0564***	-0.1342**	-0.0459***	-0.1184**
	(-3.56)	(-2.86)	(-3.84)	(-2.53)	(-3.06)	(-2.12)
County redistribution preferences	-0.0095	-0.1063				
	(-0.47)	(-1.62)				
Income inequality			0.0389	-2.1007		
			(0.10)	(-1.58)		
Metro					0.0802***	0.1286
					(3.09)	(1.56)
All control variables	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.5234	0.2255	0.5290	0.2266	0.5330	0.2309
Number of observations	20,083	20,083	21,486	21,486	22,246	22,246
Panel B: Region fixed effect regressions and state fixed effect regressions						
Social capital	-0.0525***	-0.1223**	-0.0296**	-0.1563**		
	(-3.12)	(-1.97)	(-2.49)	(-2.18)		
All control variables	Yes	Yes	Yes	Yes		
Region fixed effect	Yes	Yes				
State fixed effect			Yes	Yes		
Adjusted R^2	0.5317	0.2270	0.5357	0.2273		
Number of observations	22,246	22,246	22,246	22,246		
Panel C: County-level and firm-level omitted factors						
Social capital	-0.0435***	-0.0940*	-0.0749***	-0.1789***	-0.0741***	-0.1376**
	(-3.22)	(-1.74)	(-4.35)	(-3.14)	(-4.12)	(-2.07)
Other total pay	0.0223**					
	(2.05)					
Other equity pay		0.0126				
		(0.68)				
E-index			0.0319***	0.1367***		
			(3.62)	(4.55)		
General ability					0.0963***	0.1529***
					(8.63)	(4.17)
All control variables	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.5225	0.2217	0.5423	0.2423	0.5123	0.2036
Number of observations	19,755	19,755	17,803	17,803	13,208	13,208
Panel D: Alternate social capital measures and alternate sampling method						
CCES self-reported voter turnout	-0.0835*	-0.4639***				
	(-1.89)	(-2.98)				
Linearly interpolated social capital			-0.0875***	-0.1794***		
			(-4.68)	(-2.79)		
Social capital					-0.0753***	-0.2068***
					(-4.00)	(-2.77)
All control variables	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.5291	0.2257	0.5118	0.2143	0.5619	0.2658
Number of observations	21,894	21,894	16,891	16,891	3,851	3,851
Panel E: Using county-averages variables						
Social capital	-0.1142***	-0.3062***				
	(-3.53)	(-2.80)				
All control variables	Yes	Yes				
Adjusted R^2	0.5778	0.3033				
Number of observations	5,549	5,549				

Table 4

Regressions with instruments for social capital based on inherited cultural preferences

The sample contains 22,246 firm-year observations for the period 1993–2014. Model 1 presents estimates from the first-stage regression, where the dependent variable is *Social capital* and the instruments, namely, *Power distance* and *Masculinity-femininity*, are added to the baseline models. *Social capital* is the county-level social capital index based on data from the NRCRD at the Pennsylvania State University. *Power distance* (*masculinity-femininity*) is the county-level weighted average Hofstede's scores for power distance (masculinity-femininity), where the weights are the percentages of people with first ancestry information as reported in Census ancestry data. Models 2 and 3 present estimates from the second-stage regression. *Total pay* (*Equity pay*) is the natural logarithm of one plus total compensation (equity-based compensation) for a CEO during a given year as reported in the ExecuComp database. *Fitted social capital* is the predicted value of social capital based on the first-stage regression. Control variables include *CEO age*, *CEO tenure*, *IO concentration*, *IO total*, *Firm size*, *Return*, *Return volatility*, *ROA*, *M/B*, *Leverage*, *Cash*, *CAPEX*, *County income*, *County age*, *Education*, *Religiosity*, *Political strength*, *Population growth*, *Nonwhite population*, *Population density*, *Latitude*, *Longitude*, and *Distance to river* plus industry and year fixed effects as specified in the baseline models. All of these variables are defined in the Appendix. Estimates are based on ordinary least squares regressions with standard errors adjusted for heteroskedasticity and clustered at the county-level. *t*-statistics, in parentheses, are based on two-sided tests. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

	Model 1	Model 2	Model 3
Variables	*Social capital*	*Total pay*	*Equity pay*
Power distance	-0.0612***		
	(-4.47)		
Masculinity-femininity	-0.0507***		
	(-8.07)		
Fitted social capital		-0.1599***	-0.2324*
		(-3.26)	(-1.73)
All control variables	Yes	Yes	Yes
Adjusted R^2	0.7041	0.5311	0.2305
Number of observations	22,246	22,246	22,246

Table 5

Regressions based on a propensity score matched sample

The sample contains 1,574 treatment-control firm-year observations from 787 matched pairs for the period 1993–2014. A propensity score matched method is used to generate the sample. We rank *Social capital* annually based on the data in that year. *High social capital* equals one if *Social capital* for a firm in a given year ranks at the top quartile in that year; *High social capital* equals zero if *Social capital* for a firm in a given year ranks at the bottom quartile in that year. To generate the propensity score, we use a logistic regression with *High social capital* as the dependent variable and independent variables as specified in the baseline model. Using the predicted propensity score from this logistic regression, we match without replacement a firm-year observation with *High social capital* equal to one, a treatment observation, against another firm-year observation with *High social capital* equal to zero, a control observation. We use the caliper matching method and match within a caliper of 0.01, where caliper refers to the difference in the predicted probabilities between the treatment observation and the control observation. Panel A presents the firm and county attributes across these two subsamples. Panel B presents the estimates of OLS regressions based on the baseline models of Eq. (1) after replacing *Social capital* with *High social capital*. *Total pay* (*Equity pay*) is the logarithm of one plus total compensation (equity-based compensation) for a CEO during a given year as reported in the ExecuComp database. Control variables include *CEO age*, *CEO tenure*, *IO concentration*, *IO total*, *Firm size*, *Return*, *Return volatility*, *ROA*, *M/B*, *Leverage*, *Cash*, *CAPEX*, *County income*, *County age*, *Education*, *Religiosity*, *Political strength*, *Population growth*, *Nonwhite population*, *Population density*, *Latitude*, *Longitude*, and *Distance to river* plus industry and year fixed effects as specified in the baseline models. All of these variables are defined in the Appendix. Estimates are based on ordinary least squares regressions with standard errors adjusted for heteroskedasticity and clustered at the county-level. *t*-statistics, in parentheses, are based on two-sided tests. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

Panel A: Firm and county attributes across the subsamples

	High social capital = 1 (*N* = 787)		*High social capital* = 0 (*N* = 787)			
Variables	Mean	S.D.	Mean	S.D.	Diff	*t*-value
CEO age	56.4752	6.7080	56.4523	7.4534	0.0229	0.06
CEO tenure	7.0838	7.0946	7.3341	7.5716	-0.2503	-0.67
IO concentration	0.0272	0.0361	0.0255	0.0408	0.0018	0.91
IO total	0.6780	0.2189	0.6665	0.2182	0.0116	1.05
Firm size	7.2618	1.3777	7.2732	1.5488	0.0115	0.15
Return	0.0146	0.0328	0.0133	0.0336	0.0014	0.82
Return volatility	0.1017	0.0592	0.1038	0.0594	-0.0020	-0.67
ROA	0.0463	0.0829	0.0474	0.0902	-0.0010	-0.23
M/B	1.7996	1.0905	1.8367	1.1761	-0.0371	-0.65
Leverage	0.2362	0.1839	0.2283	0.1758	0.0079	0.87
Cash	0.1095	0.1483	0.1115	0.1423	-0.0020	-0.27
CAPEX	0.0549	0.0509	0.0557	0.0567	-0.0008	-0.29
County income	35.4429	12.911	36.3030	9.4813	-0.8601	-1.40
County age	33.6723	2.0533	33.7155	1.7618	-0.0432	-0.44
Education	27.8170	8.1034	28.5692	7.4169	-0.7522	-0.92
Religiosity	0.5681	0.1233	0.5702	0.0815	-0.0019	-0.37
Population growth	0.0087	0.0107	0.0087	0.0121	0.0001	0.03
Nonwhite population	0.2530	0.1702	0.2577	0.1091	-0.0047	-0.66
Population density	2.1437	7.6325	3.0870	5.5625	-0.9033	-1.80*
Latitude	39.5607	3.9199	39.3344	3.8254	0.2262	1.15
Longitude	-88.289	11.805	-88.508	10.695	0.2187	0.38
Distance to river	158.89	131.63	158.14	102.55	0.7513	0.12
Political strength	-17.77	72.51	-17.46	73.10	-0.3062	-0.08

Panel B: Regressions using a propensity-score matched sample

Variables	*Total pay*	*Equity pay*
High social capital	-0.0788**	-0.2304*
	(-2.26)	(-1.91)
All control variables	Yes	Yes
Adjusted R^2	0.5897	0.3019
Number of observations	1,574	1,574

Table 6

Using headquarter relocation setting to explore the relation between social capital and CEO pay

The sample contains 959 firm-year observations for 76 firms with a single headquarter relocation during 1996–2010, of which 35 firms (41 firms) have a social-capital-increasing relocation (social-capital-decreasing relocation). Panel A presents the estimates of the ordinary least squares regressions based on the baseline models of Eq. (1) after replacing *Social capital* with *After*, *Social-capital-increasing relocation*, and *After* × *Social-capital-increasing relocation*. *After* is a dummy variable that equals one if the observation is from the period after the relocation; it equals zero if the observation is from the period before the relocation. *Social-capital-increasing relocation* is a dummy variable that equals one if a firm relocated its corporate headquarter to a county with a higher level of social capital; it equals zero if a firm relocated its headquarter to a county with a lower level of social capital. *Total pay* (*Equity pay*) is the natural logarithm of one plus total compensation (equity-based compensation) for a CEO during a given year as reported in the ExecuComp database. Control variables include *CEO age*, *CEO tenure*, *IO concentration*, *IO total*, *Firm size*, *Return*, *Return volatility*, *ROA*, *M/B*, *Leverage*, *Cash*, *CAPEX*, *County income*, *County age*, *Education*, *Religiosity*, *Political strength*, *Population growth*, *Nonwhite population*, *Population density*, *Latitude*, *Longitude*, and *Distance to river* plus industry and year fixed effects as specified in the baseline models. All of these variables are defined in the Appendix. Estimates are based on ordinary least squares regressions with standard errors adjusted for heteroskedasticity and within-county clustering. *t*-statistics, in parentheses, are based on two-sided tests. Panel B presents diagnostics comparing all firm attributes and the CEO pay measures across firms with social-capital-increasing relocation and firms with social-capital-decreasing relocation in the year immediately prior to the relocation event. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

Panel A: Effects of over-time changes in social capital on over-time changes in CEO compensation

Variables	*Total_pay*	*Equity_pay*
After	0.2913**	1.3462***
	(3.28)	(5.08)
Social-capital-increasing relocation	0.1606	-0.1020
	(1.64)	(-0.41)
After × Social-capital-increasing relocation	-0.2040**	-0.5624*
	(-2.13)	(-1.68)
All control variables	Yes	Yes
Adjusted R^2	0.6711	0.3933
Number of observations	959	959

Panel B: Diagnostics of the sample firms

	Social-capital-increasing relocation = 1	*Social-capital-increasing relocation* = 0	*t*-statistic for difference in means
Variables	Mean (S.D.)	Mean (S.D.)	
Total pay	4,806.1 (5,067.6)	6,001.6 (6,656.8)	-0.84
Equity pay	2,652.1 (2,786.1)	3,515.9 (4,718.6)	-0.99
CEO tenure	5.0571 (6.2494)	4.7805 (4.0094)	0.23
CEO age	53.371 (6.4675)	56.220 (7.4884)	-1.76*
IO concentration	0.0301 (0.0168)	0.0249 (0.0140)	1.48
IO total	0.7771 (0.1829)	0.7151 (0.2004)	1.40
Firm size	7.3593 (1.3610)	7.6673 (1.2511)	-1.03
ROA	0.0404 (0.0868)	0.0391 (0.0627)	0.08
Return	0.0103 (0.0350)	0.0177 (0.0340)	-0.94
Return volatility	0.1048 (0.0532)	0.1060 (0.0416)	-0.12
M/B	1.7172 (0.6396)	1.7369 (1.1372)	-0.09
Leverage	0.2157 (0.1577)	0.2792 (0.1695)	-1.39
Cash	0.1155 (0.1642)	0.1182 (0.1644)	-0.07
CAPEX	0.0500 (0.0362)	0.0561 (0.0498)	-0.61
Number of observations	35	41	

Table 7
Social capital and the power-pay relation

Regressions in Panel A use a sample that contains 22,246 firm-year observations for the period 1993–2014. The regression models are as specified in the baseline models in Eq. (1) after replacing *Social capital* with *High SK*, *CEO power*, or *% CEO appointed directors*, and the corresponding interaction variables. *Total pay* (*Equity pay*) is the natural logarithm of one plus total compensation (equity-based compensation) for a CEO during a given year as reported in the ExecuComp database. *High SK* equals one if *Social capital* for a firm in a given year is above the sample median value; otherwise, it equals zero. *CEO power* equals one if a firm's CEO also serves as the chairperson of the board and president of the company in a given year; *CEO power* equals zero otherwise. Regressions in Panel B use a reduced sample of 14,587 firm-year observations in which the *CEO power* variable is replaced by the *% CEO appointed director* variable. *% CEO appointed directors* is the number of directors appointed after the CEO assumed office divided by the total number of directors on the board for a firm in a year. Sample attrition is due to missing information in *% CEO appointed directors*. In all models and across both panels, control variables include *CEO age*, *CEO tenure*, *IO concentration*, *IO total*, *Firm size*, *Return*, *Return volatility*, *ROA*, *M/B*, *Leverage*, *Cash*, *CAPEX*, *County income*, *County age*, *Education*, *Religiosity*, *Political strength*, *Population growth*, *Nonwhite population*, *Population density*, *Latitude*, *Longitude*, and *Distance to river* plus industry and year fixed effects as specified in the baseline models. All of these variables are defined in the Appendix. Estimates are based on ordinary least squares regressions with standard errors adjusted for heteroskedasticity and within-county clustering. *t*-statistics, in parentheses, are based on two-sided tests. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

	Model 1	Model 2	Model 3	Model 4
Variables	*Total pay*	*Equity pay*	*Total pay*	*Equity pay*
Panel A: Power as measured by executive titles simultaneously held by CEO				
High SK	-0.0484*	-0.0830	-0.0695***	-0.1437*
	(-1.74)	(-0.94)	(-2.86)	(-1.78)
CEO power	0.1217***	0.2243***	0.0822***	0.1108*
	(5.01)	(2.84)	(4.57)	(1.79)
CEO power× High SK	-0.0760**	-0.2183*		
	(-2.31)	(-1.80)		
All control variables	Yes	Yes	Yes	Yes
Adjusted R^2	0.5344	0.2309	0.5336	0.2306
Number of observations	22,246	22,246	22,246	22,246
Panel B: Power as measured by directors appointed after CEO assumed office				
High SK	-0.0466	-0.0015	-0.1008***	-0.2378***
	(-1.22)	(0.10)	(-3.72)	(-2.65)
% CEO appointed directors	0.2589***	0.8071***	0.2017***	0.5553***
	(6.09)	(6.17)	(5.06)	(4.36)
% CEO appointed directors × High SK	-0.1177**	-0.5195***		
	(-1.99)	(-2.68)		
All control variables	Yes	Yes	Yes	Yes
Adjusted R^2	0.5354	0.2279	0.5351	0.2270
Number of observations	14,587	14,587	14,587	14,587

Table 8
Social capital and likelihood of unscheduled option awards to CEOs

Model 1 uses the empirical model as specified in the baseline model. The sample contains 8,508 firm-year observations for the period 1993–2006. Attrition is due to missing observations in *CEO age* and *CEO tenure*. Model 2 uses an alternate specification where *CEO age* and *CEO tenure* are excluded. That sample contains 12,812 firm-year observations for the period 1993–2006 with 11,149 firm-years with at least one unscheduled award in a given year (*Unscheduled* = 1) and 1,663 firm-years with no unscheduled award in a given year (*Unscheduled* = 0). In both models, the dependent variable is *Unscheduled*, which equals one if a firm in a given year makes at least one unscheduled award to its CEO; *Unscheduled* equals zero otherwise. CEO option awards data are from Thomson Financial Insider Trading, where a CEO is an individual identified in the Thomson database as either the CEO or the President of the company (rolecode CEO or P). Other control variables include *County age*, *Religiosity*, *Political strength*, *Latitude*, *Longitude*, and *Distance to river* plus industry and year fixed effects. All of these variables are defined in the Appendix. Estimates are based on Probit regressions with standard errors adjusted for heteroskedasticity and within-county clustering. *t*-statistics, in parentheses, are based on two-sided tests. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

	Model 1	Model 2
Variables	*Unscheduled*	*Unscheduled*
Social capital	-0.0905**	-0.1003***
	(-2.12)	(-2.79)
CEO age	0.0012	
	(0.44)	
CEO tenure	-0.0076**	
	(-2.53)	
IO concentration	0.1026	0.2590
	(0.16)	(0.52)
IO total	-0.0417	-0.0884
	(-0.35)	(-0.94)
Firm size	-0.1173***	-0.1155***
	(-7.29)	(-8.41)
Return	0.1228	-0.3493
	(0.21)	(-0.79)
Return volatility	1.8159***	1.6812***
	(4.33)	(5.97)
ROA	0.2693*	0.0643
	(1.77)	(0.58)
M/B	-0.0463**	-0.0445***
	(-2.49)	(-2.79)
Leverage	0.3940**	0.3926***
	(2.56)	(3.17)
Cash	0.2638	0.1769
	(1.52)	(1.34)
CAPEX	-0.4170	-0.5389
	(-0.90)	(-1.52)
County income	-0.0028	-0.0008
	(-0.52)	(-0.18)
Education	0.1278**	0.0123**
	(2.00)	(2.22)
Population growth	-2.3269	-0.9103
	(-1.12)	(-0.49)
Nonwhite population	-0.3473*	-0.2620
	(-1.73)	(-1.47)
Population density	0.0001	0.0001
	(1.43)	(1.19)
Other control variables	Yes	Yes
Pseudo R^2	0.0861	0.1011
Number of observations	8,508	12,812

Table 9
Social capital and likelihood of backdated awards to CEOs

Model 1 uses the empirical model as specified in the baseline model. The sample contains 7,141 firm-year observations for the period 1993–2006. Attrition is due to missing observations in *CEO age* and *CEO tenure*. Model 2 use an alternate specification where *CEO age* and *CEO tenure* are excluded. That sample contains 11,149 firm-year observations with 3,207 firm-years for the period with at least one backdated award in a given year (*Backdated* = 1). In both models, the dependent variable is the dummy variable, *Backdated*, which equals one if the mean cumulative abnormal return (CAR) of all unscheduled awards that a firm grants its CEO in a given year ranks at the top 30th percentile of CAR in the sample; it equals zero otherwise. For a given award, CAR is the difference in cumulative daily stock price returns over two five-day windows, (-5, -1) and (+1, +5), straddling the date of the unscheduled award (day 0). CEO option awards data are from Thomson Financial Insider Trading, where a CEO is an individual identified in the Thomson database as either the CEO or the President of the company (rolecode CEO or P). Other control variables, defined in the Appendix, include *County age*, *Religiosity*, *Political strength*, *Latitude*, *Longitude*, and *Distance to river* plus industry and year fixed effects. Estimates are based on Probit regressions with standard errors adjusted for heteroskedasticity and within-county clustering. *t*-statistics, in parentheses, are based on two-sided tests. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

	Model 1	Model 2
Variables	*Backdated*	*Backdated*
Social capital	-0.0726**	-0.0469*
	(-2.23)	(-1.85)
CEO age	0.0001	
	(0.01)	
CEO tenure	0.0034	
	(1.12)	
IO concentration	-0.0993	-0.1643
	(-0.13)	(-0.31)
IO total	0.0861	0.1483**
	(0.82)	(1.99)
Firm size	-0.0905***	-0.0711***
	(-5.75)	(-6.08)
Return	-0.6079	-0.8032**
	(-1.10)	(-2.25)
Return volatility	3.7130***	3.1894***
	(9.53)	(12.05)
ROA	0.5070***	0.2822***
	(3.63)	(2.65)
M/B	0.0223	-0.0050
	(-1.37)	(-0.42)
Leverage	-0.1114	-0.1094
	(-1.03)	(-1.26)
Cash	0.0837	0.1744*
	(0.66)	(1.92)
CAPEX	0.4841	0.3742
	(1.15)	(1.18)
County income	0.0038	0.0024
	(0.98)	(0.76)
Education	-0.0019	-0.0005
	(-0.43)	(-0.15)
Population growth	0.9143	1.2290
	(0.43)	(0.79)
Nonwhite population	-0.3336**	-0.1987
	(-1.96)	(-1.39)
Population density	0.0002	0.0001
	(0.58)	(0.35)
Other control variables	Yes	Yes
Pseudo-R^2	0.0936	0.0842
Number of observations	7,141	11,149

Table 10

Social capital and likelihood of lucky awards to CEOs

Model 1 uses the empirical model as specified in the baseline regression. The sample contains 3,958 firm-year observations for the period 1996–2005. Attrition is due to missing observations in *CEO age* and *CEO tenure*. Model 2 uses an alternate specification where *CEO age* and *CEO tenure* are excluded. That sample contains 9,872 firm-year observations with about 14% of firm-years with at least one lucky award in a given year (*Luck* = 1). In both models, the dependent variable is the dummy variable, *Luck*, which equals one if a firm awards its CEO with at least one lucky unscheduled option grant award in a given year; *Luck* equals zero otherwise. Data for *Luck* are from Bebchuk, Grinstein, and Peyer (2010). Other control variables, as defined in the Appendix, include *County age*, *Religiosity*, *Political strength*, *Latitude*, *Longitude*, and *Distance to river* plus industry and year fixed effects. Estimates are based on Probit regressions with standard errors adjusted for heteroskedasticity and within-county clustering. *t*-statistics, in parentheses, are based on two-sided tests. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

	Model 1	Model 2
Variables	*Luck*	*Luck*
Social capital	-0.1057*	-0.0686**
	(-1.91)	(-2.09)
CEO age	0.0015	
	(0.36)	
CEO tenure	0.0091**	
	(2.23)	
IO concentration	-3.7183**	-0.8353*
	(-2.24)	(-1.68)
IO total	0.5061***	0.1306
	(2.93)	(1.35)
Firm size	-0.0347	-0.0364***
	(-1.49)	(-2.62)
Return	-0.2232	0.6350
	(-0.28)	(-1.64)
Return volatility	1.1727**	0.8123***
	(2.35)	(3.54)
ROA	0.0426	0.0860
	(0.14)	(0.84)
M/B	-0.1723	-0.0025
	(-0.61)	(-0.21)
Leverage	0.0721	-0.0684
	(0.37)	(-0.73)
Cash	-0.0016	0.0696
	(-0.01)	(0.72)
CAPEX	1.4120**	0.3856
	(2.19)	(1.21)
County income	-0.0108*	-0.0048
	(-1.73)	(-1.37)
Education	0.0132**	0.0101***
	(2.03)	(2.78)
Population growth	-3.4246	-1.2022
	(-1.13)	(-0.64)
Nonwhite population	-0.8487***	-0.2017
	(-3.03)	(-1.22)
Population density	0.0001	0.0001
	(1.62)	(0.97)
Other control variables	Yes	Yes
Pseudo R^2	0.0510	0.0354
Number of observations	3,958	9,872